March 1, 1951.

Dr. Reese Vaughn,
Department of Food Technology,
Hilgard Hall; U. C.,
Berkeley 4, Calif.

Dear Dr. Vaughn:

I am sorry now that I did not think to look you up last summer, when I was teaching in the Bact. Dept. in LSB, because I would welcome the opportunity to discuss with you some problems in the taxonomy of the coliforms, which we are now running into via the study of genetic recombination in E. coli.

Lately, I have been looking into the question of the "crossability" of our original standard K-12 E. coli with fresh isolates from various sources. This has been made possible by a technique which permits the rapid testing of new isolates with a minimum of individual manipulation (in particular without having to induce long series of auxotroph and other marker mutations in each stock). I find that a small but appreciable fraction of new isolates (mostly from human feces or urine), perhaps about 5% of them, can be crossed with K-12, with recombination of their natural as well as artificially induced genetic differences. The significance of this small fraction is not clear; our tests would not detect new compatibility groups in which K-12 itself does not participate.

Naturally we are interested in the taxonomic problems bearing on this work, of which two appear to be outstanding: a) whether recombination can occur between widely divergent forms, viz. coli and aerogenes, resulting in "intermediates" with various combinations of the differential characters, like those already described from divers sources, and b) whether recombination-compatibility is itself a marker of a rather restricted group of organisms, which might have other features in common. To date, a) seems rather unlikely, as all of the intercrossable cultures have conformed to a classical E. coli description [although they vary in sucrose-fermentation, and one is a lactose-negative 'paracolon'- an essentially trivial or accidental mutation]. However, we are continuing to test a large series of isolations from human and other sources, without regard to specific classification, so that a) will probably be more or less exhaustively tested. It is with respect to b) that I would like to ask your help, and I am thinking specifically of the 233 odd intermediate coliforms described by Levine and yourself in 1942, J. Bact., 44. Is this collection

still intact, and if so, could it be made available to us for recombination tests? It would be greatly appreciated if your effort in collecting and classifying such a comprehensive group of strains from diverse sources could be used to further advantage for these genetic tests. I should point out that the recombination-screening procedure has been simplified to the point that the preliminary test takes about as much time per culture as running a set of fermentation tubes.

In an entirely different connection, I have been looking for a cellobiose fermenting coliform for some biochemical work. The gumminess of aerogenes makes it rather unwieldy for our purposes, but so far, from fecal and urine material, all of the active cellobiose-fermenters have had this rather undesirable characteristic. If you could provide intermediate cultures which ferment cellobiose strongly, are not gummy on sugar containing agar (e.g. EMB), and are methyl-red-positive, I should be particularly obliged.

I don't suppose you have any copies left, but if any reprints of your papers on the coliforms are still available, I should be more than pleased to have them.

Yours sincerely,

Joshua Lederberg,
Associate Professor of Genetics

P.S. If the 1942 paper summarizes the collection adequately, it would not be necessary to diagnose each culture with more than a reference number; only a very few of the cultures could be expected, at best, to work in this system, and these could be characterized afterwards.